

Responses to AMS Reviews of Chapter 3: Global Oceans

Joel Levy, Chapter Editor

REVIEW 1

I read the Global Oceans chapter of the State of the Climate report with interest, and for the most part found it to be well-written and to the point. I think the findings are clear, and there is a good balance between documenting long-term changes and looking at issues specific to 2007. The report is a helpful reference, and I am likely to return to it in my own research.

Here are some detailed comments.

(1) Most of the climate trends throughout this report are provided without error bars. While I recognize that the style of the report is consistent, I think that the information in this report would be more helpful if error bars were included. The IPCC 2007 report, for example, has set a clear standard for reporting 2-sigma uncertainties, and it would be nice if NOAA were able to adopt a similar standard. For example, in Figure 3.5, where are the 2007 versus baseline anomalies significant? In Figure 3.6, where are the 2007 minus 2006 differences greater than the uncertainties in the heat content calculations, and is there anywhere that the differences exceed expected year-to-year variability? Similarly, in the lower panel of Figure 3.7, are the differences statistically significant compared with the annual cycle or the estimated uncertainties? Are uncertainty estimates available for Figure 3.8? One can ask similar questions about Figures 3.12, 3.13, 3.14, 3.17, 3.19, 3.21, 3.23, and 3.27.

Response: Rejected. We agree, in principle, with the reviewer that it would be desirable to report uncertainties and to indicate where anomalies are statistically significant. In many instances, assessing the errors is a major endeavor, which, in some cases is underway but incomplete. Given the late date and the absence of time to perform new analyses or create new figures, it is impractical to accommodate this request for this publication cycle. However, this suggestion should be carefully considered for next year's report

(2) The discussion of Figure 3.3 in lines 125-128 was unclear to me. Why does the weekly OI not resolve coastal SST gradients?

Response: Accepted. Explanatory text added to the document.

(3) In line 348, as a reader I would be helped by either a little clarification in the text about what "trough interactions and small vertical shear" represent, or by a reference to an appropriate citation. For example, since this is the ocean chapter of this report, readers unfamiliar with the topic may wonder whether troughs are in the ocean or only in the atmosphere?

Response: Accepted. Citation added to document.

REVIEW 2

I've read through the NOAA annual state of the climate report. I looked through all the sections, but only have a few comments, as the report seems pretty complete.

1) The overview (say, around line 17) and SST section (line 94) both describe a "moderate-strength La Nina by the fall of 2007." However the La Nina continued to develop, and it would be hard to say it was only moderate strength by the end of 2007. Around line 96 it says weak La Nina conditions started in August, and continued to strengthen through December. Although none of this is technically incorrect -- in fall, the La Nina was only moderate strength and the La Nina started out weakly, in August -- it seems to me it underplays the strength of the actual event that developed, even if you cut off at the end of 2007. It also seems at odds with the later parts of the report, where an entire variety of fields are referenced to the development of the La Nina. I would change this early wording to more accurately reflect the strength of the La Nina event that developed.

Response: Accepted. The term "moderate" is retained in line 94, but the term "moderate-to-strong" is inserted in line 97. NOAA has a definition for La Nina, which is keyed on the Oceanic Nino Index (ONI) defined as the three-month running-mean SST departures in the NINO3.4 region. The ONI was derived from the Extended Reconstructed SST version 3. However, NOAA's "ENSO Diagnostic Discussion" also mentioned the strength of La Nina from time to time. Although there are no strict definition for strength, "weak" is often referred to when ONI is larger than -1C, "moderate" when ONI is less than -1C, and "strong" when ONI is less than -1.5C. The monthly ONI has been

2007	8	-0.50
2007	9	-0.87
2007	10	-1.38
2007	11	-1.45
2007	12	-1.50
2008	1	-1.81
2008	2	-1.86
2008	3	-1.14

Therefore, we may say a weak La Nina developed in August 2007, and then grew rapidly into a moderate-to-strong La Nina in fall/winter 2007.

2) By the last quarter of the year, a pretty classic negative Pacific Decadal Oscillation pattern had been set up in the North Pacific. Since that has an influence on weather over North America, I would have mentioned this somewhere, such as in section 3.b.1.

Response: Accepted. Text and a link to a web site showing the PDO Index have been added to the document.

3) My sense is that the ocean acidification problem has been under-appreciated until recently. I would not be surprised to see the subject, which is treated in a sidebar in the current version, expanded to more equal coverage in future reports.

Response: Accepted. We agree. No change requested or made.

REVIEW 3

Review of NOAA's annual State of the Climate 2007 for BAMS **Section 3.d.1**

Surface Current Observations – R. Lumpkin and G. Goni

General comments: It is evident that this and all other subchapters were given stringent page limitations, and accordingly this is a very brief descriptive summary. Nevertheless, I found the text to be somewhat tedious, in spite of the brevity. I think the authors could communicate the results more effectively and hold the reader's interest by listing three or so highlights for 2007 at the start. My suggestions are: 1. Westward equatorial Pacific current anomalies and the link to La Nina conditions developing during the year 2. Long term trends in EKE. 3. Trend in the Brazil-Malvinas separation latitude. I see that J. Levy has already selected the same themes for highlight in the overview, and I think it will strengthen the article to highlight them again here.

Outline recommendations:

1. Start with a new short paragraph of the highlights noted above.
2. Label the next section: "Data and analysis" and include the two paragraphs on the first page (lines 554-571).
3. Move the heading "Global Overview" to line 576.

Response: Accepted. Changes 1-3 made, as requested.

Detailed comments:

1. In situ data (lines 554-562): After this initial description, I see neither mention of these data sets nor any figures. Are they used at all in the subsequent assessments?

Response: Due to the brevity of this report, methodologies are simply cited without further elaboration. This is consistent across the report.

I would recommend that at least showing a low-pass filtered record of a TAO current record from the central equatorial Pacific, co-plotted with Nino 3.4 SST, would provide a good illustration of the evolution during the year described in so many words in the Pacific subsection. A "spaghetti plot" of all the drifter trajectories for the year would also be illustrative.

Response: Rejected. While the editor and the authors agree with the reviewer, we wish to note that the editor previously removed such figures because of space limitations.

Although I am a strong advocate for satellite altimetry, the presentation here, in my view, underutilizes NOAA's considerable investment in *in situ* measurements. Needed somewhere is a statement such as "*The climate assessments presented here are based on our combined evaluation of mooring, drifter and satellite-based surface current measurements*".

Response: Accepted. Text inserted as requested with a minor alteration to the wording.

2. Current maps and use of NCEP winds (lines 566-570): Here they refer to a synthesis of various measurements without explaining any of their methods.

Response: Again, this report, which is only long enough to summarize conclusions, does not elaborate on methods.

Despite the reference to NCEP wind data and the reference to Niiler (2003) Ekman model, all the plots show only derived geostrophic currents estimated from altimetry. So what value is there in mentioning NCEP winds here?

Response: NCEP winds are used to remove the Ekman component.

Furthermore, Niiler et al (2003) Eq. (1) is not valid at the equator ($f=0$). The authors do not describe how they have computed equatorial geostrophic velocities, as evidently they must have in Figs 3.13 and 3.15. I see no references to their methodology nor to any prior scientific results using satellite based global currents. This is essential for validating their descriptions, especially of the zonal equatorial current anomalies and the 2007 La Niña in the **Pacific Ocean** subchapter of this report. A treatment of the equatorial singularity for both wind-driven and geostrophic currents is given by Bonjean and Lagerloef (2002, JPO), and the validation of satellite-derived currents against drifters at various latitudes is described in Johnson et al (2007, JTech), and also updated at <http://www.oscar.noaa.gov/>. Lastly, the absence of any reference or citation to the NOAA-sponsored OSCAR satellite-based surface current dataset seems to be a significant omission. Monthly analyses based on the OSCAR appear in NOAA's Monthly Climate Diagnostics Bulletin, and there are quite a few publications in the literature in the past decade that have utilized the OSCAR data. At the very least, the authors could cite some of this prior work to indicate that their own mapping methodology has a valid basis and precedent in the literature.

Response: Accepted. A sentence has been added to the text citing two methodological papers. The reviewer is reminded that this report is focused on summarizing findings; the interested reader is referred elsewhere for methodological details.

3. Baseline for Anomalies (Line 570): The reference period they use for anomalies, 1993-1998, is short (5 years) relative to the record length of altimetry (15 years) and will be biased by the overwhelming 97-98 Niño. What is the rationale for this choice? A much longer period, say, until 2006 or 2007, is recommended.

Response: Rejected. The analyses use the near-real time gridded AVISO product, and therefore the AVISO time period. The AVISO user manual states the following: AVISO/Altimetry 1996, "AVISO User Handbook for Merged TOPEX/POSEIDON products", AVI-NT-02-101, Edition 3.0 and say that the anomaly is defined as "January

1993 to January 1999 (AVISO/Altimetry, 1996)." The text already states clearly that this IS the publicly available AVISO product being used, and even gives the URL.

4. Pacific Ocean (Lines 598-631): The correlation and phasing between Pacific equatorial surface current anomalies and El Niño/La Niña SST anomalies has been described by Lagerloef et al (2003, GRL). This discussion could be enhanced with a time-series plot of some type of surface current index and SST indices (as suggested above). Figure 3.15: a vector plot of surface current anomalies or drifter trajectories superimposed on an SST anomaly map would be more interesting than this figure.

Response: Rejected. We regret that the time available for responses to reviews is inadequate for creation of new analyses and figures.

Lines 628-630, suggested re-wording: *The longterm trends in EKE reveal large, positive values in the Kuroshio Current and Kuroshio Extension region (Fig. 3.14), extending to approximately 170°W, which may be related to the regional intensification of the surface currents (Fig. 3.13).*

Response: Accepted. Change made.

5. Atlantic Ocean, Figure 3.16: Please add a data point (circle) for the 2007 mean annual value.

Response: Accepted. Change made to figure.

References

- Bonjean, F., and G.S.E. Lagerloef, 2002: Diagnostic Model and Analysis of the Surface Currents in the Tropical Pacific Ocean. *J. Phys. Oceanogr.*, **32**, 2938–2954.
- Johnson, E.S., F. Bonjean, G.S.E. Lagerloef, J.T. Gunn, and G.T. Mitchum, 2007: Validation and Error Analysis of OSCAR Sea Surface Currents. *J. Atmos. Oceanic Technol.*, **24**, 688–701.
- Lagerloef, G.S.E., R. Lukas, F. Bonjean, J.T. Gunn, G.T. Mitchum, M. Bourassa, and A.J. Busalacchi, 2003: El Niño Tropical Pacific Ocean surface current and temperature evolution in 2002 and outlook for early 2003. *Geophys. Res. Lett.*, **30** (10), 1514, doi:10.1029/2003GL017096.

Section 3.c Sea Surface Salinity – G.C. Johnson and J.M. Lyman

1. A very interesting presentation. Referencing Argo to WOA 2001 provides a promising indicator of decadal scale trends.

Response: Accepted. We agree. No change requested or made.

2. Line 465: The Aquarius mission will launch in 2010 (the website is being updated).

Response: Accepted. Change made.

3. Figure 3.11: While the patterns are interesting, they are still a bit mottled. Perhaps adding a meridional section of zonal average anomalies would be more definitive. Also, averaging 2005 – 2007 together and then computing the anomaly to WOA 2001 may also provide a less noisy map.

Response: Rejected. The authors and editor appreciate the good suggestion, but feel that this query may be better treated in a separate scientific manuscript. Unfortunately, the time constraints on submitting revisions are simply too short to implement this suggestion in a sufficiently careful manner.

Section 3.e Sea Level Variations – M. A. Merrifield, S. Gill, G. T. Mitchum, and P. L. Woodworth

1. Lines 808-809: It should be emphasized here and in the Overview by J.M Levy (Lines 52-56) that, while the global sea level change from 2006 to 2007 was 1.1 mm, 2006 had a much greater change, and the two-year increase from 2005-2007 was ~7.1 mm was consistent with the 15-year trend of ~3.4 mm/year.

Response: Accepted. Text added to the document.

2. Lines 808-817: This entire paragraph seems out of place and would fit better if moved to line 783.

Response: Accepted. Change made.

3. Lines 784-791: I recommend cross-referencing the Niño-Niña discussions here with the Surface Currents subchapter (lines 590-620) and the SST section (lines 92-99), and vice versa, to make the whole report more cohesive.

Response: Accepted. References to Figure 3.2 (SSTs) and Figure 3.15 (surface currents) inserted in the text of this section, and reverse references submitted in the SST and surface sections. Thank you for the good suggestion.

REVIEW 4

The Global Ocean section of the 'State of the Climate report' does a fine job of documenting the state of various aspects of the ocean pertinent to climate, with comparison to the climatic state in general and to 2006 in particular. These yearly documentation reports are of great value to the community.

The authors for the most part just present the facts, backed up by useful figures. Though some do provide a bit of analysis [its so tempting], others stick to the facts. This leads to some unevenness in the presentations. I found that the CO2 section presented a nice balance. The author expresses his appreciation.

Some sections [eg surface current] have sub-titles, as Pacific, Atlantic, Indian Oceans, other don't. I'm not so certain that the effort to produce a more even product [in terms of

subtitle and balance of facts to analysis] would enhance the documentation value of the report, and so I'm not advocating this. [No change requested or made.](#)

Mention of aspects of the condition of specific features [e.g. Agulhas Current and other western boundary currents] in many sections, tell of an important trend. Cross citations between the sections might help bring it all together. But then again this is an analysis task, while the purpose of the report is documentation, so OK not to delve into the causes and consequences of the 2007 anomalies. Comparison of sea level to SST and SSS might be interesting. Perhaps in future reports there can be a more integrating overview statement of the interconnection of the facts brought out by each of the sections. [No change requested or made.](#)

The summary statement in leading pages of the report helps weave the report together. The abstract does a nice job of selecting [in bullet fashion] one of the more relevant aspects of 2007 from each of the sections. Some sections might deserve two or three bullets. I suggest, perhaps in future reports, a more expansive summary. [No change requested or made.](#)

Perhaps change 'Latent evaporation' to 'Evaporative heat loss'.
[Response:](#) Accepted. [Change made.](#)

As many readers may check out the figures. It might be a good idea to spell out the various abbreviations given in those captions, so the figures can 'stand-alone'
[Response:](#) Rejected. [This is a report-wide issue. We wish to keep captions brief.](#)

Some captions refer to a 'figure 1'. I think they mean the first figure of the authors' section, e.g. fig 3.2, 3.6]
[Response:](#) Accepted. [Changes made.](#)

REVIEW 5

Review of BAMS NOAA state of the climate report for 2007: Oceans Section 3.a-g

This annual report summarizes changes in a wide variety of ocean variables related to the climate system. These include SST, heat content, surface fluxes, tropical cyclone heat potential, sea surface salinity, current anomalies, the meridional overturning circulation, sea level, pCO₂ and chlorophyll. Each section will be discussed in turn.

SST –

High northern latitude warming noted in both Pacific and Atlantic for 2007. the discussion of a waning El Nino and onset of La Nina seems a little confused, in that the initial focus is on El Nino, which disappeared quickly in 2007 to give way to La Nina. I would simplify this section and just discuss La Nina. Finally the comparison of daily and weekly SST anomalies seems of only regional interest, and little can be developed on it here. If space is an issue this bit might be cut (Fig 3.4).

Response: Rejected. The reviewer is correct that El Nino abated quickly, but we believe that mention of it is appropriate because readers wish to know about the phenomenon. Also, we decided to retain the comparison of daily and weekly anomalies because it demonstrates the relationship between resolution and anomalies, i.e., between analyses and the applications they address.

Heat content –

The analysis in terms of OHCA in terms of surface heat fluxes is useful, especially emphasis on ocean advection. Paragraph starting at line 202 seems a bit confusing, in that OHCA indicated cooling 2006 – 2007 in northern North Atlantic whereas SST indicates warming here. Perhaps it would be best if all authors stuck to anomalies against a baseline instead of year to year differences. Or do both but do them consistently.

Response: Accepted. The author has modified the text in this paragraph to better distinguish discussions of 2006 to 2007 changes versus the 2007 anomalies from the baseline period. He also added a sentence to an earlier paragraph explicitly calling attention to visual similarities between Fig. 3.1 and Fig. 3.5 (but not 3.6) on a global scale, since corresponding features are not limited to the N. Atlantic.

Heat fluxes –

line 240-241, Substitute “..Most of the solar energy absorbed by Earth is taken up in the top layers of the ocean.” for the existing sentence.

Response: Accepted. Change made.

Line 314, substitute “caused” for “forced” in “...Atlantic SST forced by the atmospheric forcing?”

Response: Accepted. Change made.

Line 339, insert “the” in front of “global water cycle”.

Response: Accepted. Change made.

Tropical Cyclone heat potential –

line 364 substitute “growth” for “intensification” in “intensification of intense TCs..”.

Response: Accepted. Change made.

Please explain the likely contribution of the ARGO float array to TC heat potential.

Also, how does the complete lack of ARGO floats in the Caribbean and Gulf of Mexico affect TC in these crucial regions?

Response: Rejected. While the reviewer’s question is an interesting one, the report focuses on analysis of existing data sets and does not delve into observing system design.

Sea Surface Salinity –

Line 465 Change 2009 to 2010 for Aquarius launch date.

Response: Accepted. Change made.

Line 491, please clarify if WOA2001 is for year 2001 alone or climatology for

all prior years.

Response: Accepted. Change made. WOA2001 is a long-term climatology made using quality controlled data from all available years prior to its construction. The author has inserted the descriptor “long-term” before “climatology” in the first three paragraphs of this section to make this clear to the reader.

Line 500, reference the Yu and Weller evaporation increase from the heat flux section.

Response: Accepted. Change made. The reference is appropriate and has been made in the text. The author thanks the reviewer for pointing it out.

Surface Current Observations –

Line 617; add “in surface temperature” after “sharp drop”.

Response: Accepted. Change made.

Meridional Overturning Circulation –

Line 721; delete “within one year” after “In fact”.

Response: Accepted. Change made.

Sea Level Variations –

line 770; Explain why RSL data does not have inverse barometer correction.

Response: Reject. That is, we are not making a change to the text of the document, since this is rather a fine point. The reviewer, however, has asked for the explanation, which follows: The inverse barometer (IB) correction has been applied to the SLA but not to the RSL, primarily due to the lack of local atmospheric pressure measurements at many of the RSL stations. The IB correction is small on annual time scales, hence the strong correspondence of the mean SLA and RSL fields presented, and the authors prefer to include the influence of atmospheric pressure in the analysis of RSL extremes.

Line 777; add “sea level” after “..above average.”

Response: Accepted. Change made.

Global Ocean Carbon Cycle –

Lines 872-875. the northern North Atlantic also has very deep mixed layers where vertical mixing is a likely control on pCO₂. Can you explain why the correction was only done in Bering Sea and Southern Ocean?

Response: Accepted. Changes made to text in document to better clarify the rationale for applying the correction to the Bering Sea and the winter high latitude Southern Ocean. As denoted in the citation in the document, these are areas where vertical mixing is a stronger control on surface pCO₂ values than the atmospheric uptake so no time dependent adjustment was made (Takahashi et al., 2007). Although the North Atlantic also has high mixing, observations indicate that surface water pCO₂ is increasing at the same rate as the atmosphere. It is not completely clear why there is a difference. It could be that the mixing is not as deep in the North Atlantic, or the fact that the vertical CO₂ gradients are very different, or the interplay between the biological and physical controls

are different. This is still a matter of some research but the bottom line is that the corrections were based on observed trends from measurements made over several decades.

Line 932; Delete “Despite these shortcomings..” The models have far more shortcomings than the data and are hardly a standard of comparison!

Response: Rejected. As the carbon time series observation network grows in space and duration more measurements will become available for comparison, but at this point the numerical models are all we really have for the evaluation. As discussed in the text, an empirical fit is used to extend the data in time and space. Accordingly, the authors feel that it is very relevant to compare these estimates with the only other available estimates, which are numerical models.

Ocean Acidification –

This seems a little alarmist, lots of potential “negative impacts” are suggested, but few references are given. I suggest augmentation with more references to substantiate the claims.

Response: Accepted. Change made. Two citations added.

Global Ocean Phytoplankton and Productivity –

Line 1110-1111; some elaboration on Polovina et al (2008) is required. Is this a modeling study, data study or what?

Response: Accepted. Change made to text in the document.

This section is rather high on the “self-citation index”. Perhaps that can be toned down a bit.

Response: Accepted. Several citations have been removed.